

# A Test of the Rational Electoral-Cycle Hypothesis<sup>\*</sup>

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## Abstract

This paper develops a three-step empirical methodology to test the rational electoral-cycle hypothesis. The first step consists of testing for the existence of electoral cycles in fiscal policy. The second step conducts three tests for how such cycles should depend on election outcomes as suggested by recent political agency models. The third step is to regress electoral success on fiscal policy. This three-step approach is applied to a panel of Swedish local governments with more than 2000 observations from elections. The findings are as follows: (i) spending is raised and taxes are cut in the election year, (ii) in the election year, spending is higher for a government that will be re-elected as compared to those that will not be re-appointed, (iii) in the post-election year, spending is higher and taxes are lower for re-elected governments than for newly elected ones, (iv) re-elected governments spend less and tax more in the post-election year as compared to the election year, (v) conditional on taxes, spending is positively related to electoral success. These set of findings are consistent with Rogoff's equilibrium budget cycle model where a government signals its competence through cycles in fiscal policy.

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## 1. Introduction

The electoral-cycle models by Nordhaus (1975) and Lindbeck (1976) predict that politicians should manipulate economic policy just before elections to increase their chances of re-election. While politicians are frequently characterized as driven by such office-seeking motives, surprisingly little systematic evidence supports the electoral-cycle hypothesis.<sup>1</sup> In recent years, however, some studies claim to find support for an electoral cycle in fiscal policy.<sup>2</sup> These findings then raise the deeper question of why such policy cycles should help politicians to get re-elected. The early literature sidestepped this issue by assuming that voters had inconsistent or irrational expectations.

The recent political agency literature proposes explanations of electoral cycles based on voter rationality. At the heart of this literature is the conflict of interest between citizens and politicians and the main incentive mechanism available to discipline politicians is through the act of voting. Rational but uninformed voters reward good performance in office with their vote, because they attribute good performance either to competence of the politician or to restraint in the use of political power. In other words, election is a mechanism for curbing moral hazard problems and to sort in politicians with desirable characteristics. That voter's appraise their politicians by using such a retrospective voting scheme suggests that the incentives for a politician to appear competent and to perform well are stronger just ahead of elections.

While the models by Lindbeck and Nordhaus predict electoral cycles in fiscal policy before elections, the political agency models offer much sharper predictions about how such cycles should vary with electoral outcomes. There are basically two different classes of political agency models as represented by Rogoff (1990) and Besley and Case (1995) and Banks and Sundaram (1998). Rogoff emphasizes reputation building by office seeking politicians, whereas Besley and Case/Banks and Sundaram stress electoral accountability of rent-seeking politicians. This difference in politicians' motivations has

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<sup>1</sup> For example, Alt and Crystal (1983) in their review of the state of the literature 20 years ago conclude,<sup>1</sup> "No one could read the political business literature without being struck by the lack of supporting evidence." See Drazen (2001) for a recent review.

<sup>2</sup> The recent studies are: Alesina et al. (1997) with data from OECD countries, Blais and Nadeau (1992) and Reid (1998) with data from Canadian Provisional governments, and Bizer and Durlauf (1990) with data from the US. Shi and Svensson (2002) also find evidence of opportunistic manipulation of fiscal policy instruments using a cross-country data set from both developed and developing countries.

implications for the predictions about the fiscal behavior of politicians. While both types of models have the same prediction about taxes, i.e., good politicians are associated with lower taxes, their prediction for spending differs. In Rogoff's model higher spending is associated with good politicians, while the opposite is true in Besley and Case/Banks and Sundaram's set-ups.

The predictions from the agency models can be tested by comparing the fiscal choices of politicians across four possible states of the world: election year about to win, election year about to lose, post-election year re-appointed and post-election year newly elected. One test is that fiscal behavior of politicians in the election year should differ depending on whether they will be re-elected or not in the upcoming election. This test has to do with voters adopting a "cut-off" rule (i.e., rational retrospective voting) under which politicians are re-elected only when policy outcomes exceed a critical bound and good politicians' being able to separate themselves from bad ones. A second test is that the fiscal behavior of policymakers in their post-election year should differ depending on whether they have been re-elected or are newly elected, which is due to a selection effect: only good politicians get re-elected while only some of the newly elected politicians are good. A third test is that the post-election year fiscal behavior of re-elected politicians should differ from their election year behavior, which is based on that re-election incentives are stronger just ahead of elections than afterwards.<sup>3</sup>

To the best of my knowledge, only two studies have empirically explored any of these tests. Bizer and Durlauf (1990) show that taxes are reduced two years prior to successful presidential re-election attempts, and Besley and Case (1995) present evidence that Democratic governors change their fiscal behavior when they are in their second term and face a binding term limit as compared to their first term in office. Hence, Bizer and Durlauf shed light on the first test,<sup>4</sup> whereas Besley and Case perform the third test since they compare the fiscal choices of an incumbent politician when she faces a re-election incentive versus when she does not. Despite that these studies have provided some useful information about the practical relevance of political agency models, our

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<sup>3</sup> Although, politicians may serve for a number of periods and may therefore care for their reputation, it is reasonable to think that the strength of the re-election incentive should vary with the time left to the next election.

<sup>4</sup> Bizer and Durlauf do not directly perform the first test since they do not make an explicit comparison with unsuccessful presidential re-election attempts.

knowledge of the real world relevance of the rational electoral-cycle hypothesis is still quite limited.

The contribution of this paper is to fill this lacuna by using a three-step empirical methodology applied to an attractive data set from Swedish local governments with 2259 observations from election periods. In the first step, I will test for an election year effect in fiscal policy. In the second step, I will perform the tests derived from the political agency literature as discussed above. In the third step, I will regress electoral success on fiscal policy. The benefit of using this three-step methodology is that we are able to get a coherent picture of all working parts of the rational electoral-cycle hypothesis.

There are some other attractive features of using Swedish local governments as a testing ground other than the very large number of observations from elections. Importantly, the source of variation used for identifying an electoral cycle in fiscal policy comes from an exogenously fixed election schedule, which avoids any endogeneity problems<sup>5</sup> associated with that incumbents may strategically choose when to call an election.<sup>6</sup> Elections are always held on the third Sunday of September every fourth year.<sup>7</sup> Another attractive feature is that the fiscal year is the same as the calendar year, which avoids bias due to that the fiscal policy variables is not synchronized with the election cycle.<sup>8</sup> Swedish local governments also have the constitutional right of self-government,

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<sup>5</sup> One might argue here that the estimate of the electoral effect in the case that the incumbent can call an early election would be biased downwards since it is typically assumed that an incumbent is only going to call an early election when the economy is booming. However, this statement is only going to be true if fiscal policies are counter cyclical and if the only reason for calling early elections is due to the state of the economy. Lane (2002), for example, finds empirical evidence of both pro and counter cyclical fiscal policies, which makes it hard to predict the overall sign of the bias. More generally, the direction of the bias is quite difficult to assess under less restrictive assumptions about the reason to call an early election and the correlation between the omitted variables and the policy outcome of interest. Moreover, one must use an instrumental variable approach if elections and policy outcomes are determined simultaneously in order to get consistent estimates. The bottom line of the above discussion is that we need to understand the source of variation used to estimate the parameter of interest in order to make causal inference, a general line of argument that has been forcefully emphasized by labor economists in the natural experiment approach, as discussed by Angrist and Kreuger (1999).

<sup>6</sup> Among the OECD countries, for example, it is only in Norway, Sweden, Switzerland, and the U.S. the incumbent government is unable to call an early election. Among the other OECD countries with flexible election calendars, 44 percent of all elections during the period 1961 to 1988 were held at least one year before the current government's term was due to expire and 53 percent at least six months early (Mackie and Rose 1991) thus making the calling of an early election a quite prevalent feature.

<sup>7</sup> Elections used to be held every third year, but as from 1994 elections are held every fourth year.

<sup>8</sup> The work on cross-country data, for example, face this problem when having to define the election indicator depending on whether the election is held early or late during the election year.

they have no restriction on borrowing, and they have no balanced budget rules.<sup>9</sup> Moreover, only 20 percent of their revenues are from grants, whereas the bulk comes from a proportional income tax, which each municipality can set freely.<sup>10</sup> In other words, they have a large degree of freedom in fiscal policy, which has resulted in quite large differences in policy outcomes across local governments. Finally, Swedish local governments also play a significant role in the Swedish Economy. During the sample period their total spending amounted to 20-25 percent of GDP and they employed 20-25 percent of the total Swedish workforce. This makes them more economically significant than most other sub-national governments around the world.

The remainder of the paper is organized as follows. The next section presents two classes of political agency models and their implications for how electoral cycles in fiscal policy depend on election outcomes. Section 3, develops the three-step empirical methodology used for testing the rational electoral-cycle hypothesis, while section 4 describes the data from Swedish local governments. Section 5 presents the results and section 6 concludes.

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<sup>9</sup> As from 2000, there exists a balanced budget rule.

<sup>10</sup> From 1991 to 1993, however, the central government imposed a temporary tax cap.

## 2. Rational electoral-cycle models

In this section, I present and discuss two classes of political agency models as represented by Rogoff (1990), Besley and Case (1995), and Banks and Sundaram (1998). Rogoff's model emphasizes reputation building by office seeking politicians while Besley and Case/Banks and Sundaram's models stress electoral accountability of rent-seeking politicians. These differences will have implications for the predictions about fiscal policy as discussed below.

### ***Rogoff's model***

In Rogoff's model an incumbent politician must choose levels of public goods, taxes and public investment. The level of public investment is observable with a lag while taxes and public goods are observable immediately. Elections take place every other period, but in each period, there is a shock to competence observed only by the incumbent. Politicians differ in their competence where high competence means that they can produce a higher level of public spending for a given level of taxes. Competence evolves according to a MA(1) process, which implies that competence in any given period contains both a contemporary portion and the realization of the preceding period. The value of the competence shock becomes known to voters with a one period lag. Thus, when an election comes, voters observe the off-election year competence shock but not the election year value. Rogoff assumes that politicians have the same utility function as voters but they also get some independent level of utility from holding office, i.e., ego rents.

This set up implies that competent politicians would like to reveal to voters that they are competent since they care about voters' welfare. The competent incumbent will signal her competence just before elections by boosting spending and cutting taxes. A competent policymaker is able to do this since it is less costly for her to cut back on public investment in order to finance a given level of public spending (holding taxes fixed). In Rogoff's model, competent politicians are therefore responsible for the electoral cycle in spending and taxes. If there were no re-election possible, then no cycle would occur. Thus, Rogoff's model has implications for fiscal policy choices conditional on election outcomes as follows:

PRE-ELECTION PREDICTION: In the election year, voters re-elect those politicians that provide higher level of public good spending and a lower level of taxes since this is a signal of high competence.

POST-ELECTION PREDICTION: After the election, re-elected politicians provide higher level of public good spending and lower levels of taxes than untried politicians because they are on average more competent

PREPOST-ELECTION PREDICTION: After the election, re-elected politicians cut back on the level of public spending and increase taxes as compared to their election year level since the re-election incentive is weaker (i.e., no need to signal)

### ***Besley and Case/Banks and Sundaram's model***

The agency models by Besley and Case (1995) and Banks and Sundaram (1998) do not explicitly model the public finance problem, but their models could be extended to such an analysis where good politicians do less of rent-seeking than bad ones.<sup>11</sup> In fact, the emphasize of the role of elections as a mean to curb rent-seeking is an implicit assumption of Besley and Case analysis, and which will affect the implications for electoral cycles in spending since higher levels of public spending will be associated with bad politicians, in contrast to the prediction from Rogoff's model.

Besley and Case's model is basically a stripped down version of Banks and Sundaram's model where politicians only can stay in office for two periods, which can be interpreted as model of repeated elections with the presence of term limits or that the second period represents an off-election year when the re-election incentive is not effective, as in Rogoff's model.<sup>12</sup> These models assume that politicians differ in their types and that their actions (i.e., amount of rent diversion) are unobserved; assumptions they also have in common with Rogoff's set up. It is also assumed that voters make inference based on an observed payoff, which depends on the action the politician takes, and use this information to update their assessment of her type. Actions preferably to voters are associated with higher costs for the politician. Incumbents who displease voters by extracting excessive rents are removed from office. In other words, voters use a

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<sup>11</sup> See Besley (2003) for such an extension.

<sup>12</sup> Rogoff assumes that competence follows an MA(1) process which implies that competence is uncorrelated across electoral cycles and therefore the incumbent re-election incentive is not effective in off election periods.

retrospective voting scheme and reappoint the incumbent only when the reward she generates exceeds a critical bound. This type of rational retrospective voting rule is a feature of the Rogoff's model as well. Banks and Sundaram show that there exist equilibria which possess the following properties: politicians' strategies are ordered in type in the sense that better politicians take higher actions and that voters use a cut-off rule where politicians are re-elected only if the election period reward exceeds a critical amount (i.e., Proposition 3.1 in Banks and Sundaram), politicians of each given type take higher actions in the election year than off-election years (i.e., Propositions 3.2 and 3.3), and re-elected politicians work harder than newly elected politicians (i.e., Proposition 3.4). If we now make the assumption that higher types of politicians extract less rent than lower types, we will get the following predictions about electoral-cycles in fiscal policy and electoral outcomes from the Besley and Case/Banks and Sundaram type of models:

PRE-ELECTION PREDICTION: In the election year, a politician that increase spending and raise taxes is not re-elected since voters' associate higher levels of spending and taxes with more rent extraction.

POST-ELECTION PREDICTION: After the election, re-elected politicians do less rent-seeking than untried politicians since they are on average better. Hence, spending and taxes are lower for re-elected politicians than newly elected ones.

PREPOST-ELECTION PREDICTION: After the election, re-elected politician extract more rents as compared to their pre-election level since the re-election incentives are weaker. Thus, re-elected politician increase spending and taxes in the post-election year as compared her pre-election level.

### ***Test of rational-electoral cycle models***

To test the predictions from the agency models we need to compare politicians' choice of fiscal policy across four possible states of the world: election year about to win, election year about to lose, post-election year re-appointed and post-election year newly elected. Table 1 show the definition of four indicator variables each representing one of these four states of the world. With these indicators we can test the implications from both agency models by performing three different tests. These tests are displayed in Table 2, where



PRE-ELECTION is a test whether the policy choices of politicians in the election year differs depending on whether they will be re-elected or not in the upcoming election, POST-ELECTION is a test whether the policy choices in the post-election year differ depending on whether the politicians have been re-elected or if they are newly elected, and PREPOST-ELECTION is a test whether the fiscal policy choices of re-elected politicians differ in the post-election year as compared to the election year. The predictions from the agency models and how they relates to these three tests are presented in Table 3, which reveals that the predictions from the two types of agency models about fiscal policy choices only differs for public spending.

### 3. A three-step empirical methodology

As discussed in the introduction, a three-step empirical methodology will be used to shed light on all working parts of the rational electoral-cycle hypothesis. The first step is to test for electoral effects in policy. The second step is to test the more specific predictions about fiscal policy choices and electoral outcomes as suggested by the political agency models in the previous section. The third step is to regress electoral success on policy.

#### Step 1

To test for electoral effects in fiscal policy, I will estimate regressions of the following types

$$(1) \quad P_{it} = \mu_i + \delta ELE_{it} + \gamma P_{i,t-1} + X_{it}\theta + \varepsilon_{it}$$

$$(2) \quad \Delta P_{it} = \mu + \pi ELE_{it} + \Delta X_{it}\beta + u_{it}$$

where  $i$  indexes municipalities and  $t$  index time since we are using a panel of Swedish local governments.  $P_{it}$  is total spending or a proportional income tax rate (see section 4 below),  $ELE_{it}$  is an election year indicator variable defined as 1 if an election year and zero otherwise,  $X_{it}$  is a set of control variables and  $\mu_i$  is a fixed municipality fixed effect. The parameters of interest are  $\delta$  and  $\pi$  since they measure the election effect.

Equation (1) is a dynamic specification with a lagged dependent variable as used by many previous studies in the political business cycle literature, but it also includes fixed municipality fixed effect to take into account any unobserved heterogeneity across municipalities. The inclusion of a lagged dependent variable together with fixed effects can create potential estimation problems, but considering the rather long time period

( $T=25$ ) the potential bias of using a fixed-effect (FE) estimator is probably small.<sup>13</sup> Nevertheless, I will use an alternative instrumental variable estimator as developed by Anderson and Hsiao (1982) that may be more attractive in a dynamic panel data context with a small  $T$ . This estimator is constructed by first differencing equation (1), i.e.,  $\Delta P_{it} = \delta \Delta ELE_{it} + \gamma \Delta P_{i,t-1} + \Delta X_{it} \theta + \Delta \varepsilon_{it}$ , and then applying an instrumental variable method using  $\Delta P_{i,t-2}$  as an instrument for  $\Delta P_{i,t-1}$ .

An alternative way of specifying a dynamic model is via the error term rather than using a lagged dependent variable. This is the method used to estimate equation 2. With the exception of the election indicator variable, all the variables are first-differenced.<sup>14</sup> The reason for not first-differencing the election indicator variable is that it would imply a model where all election increases in policy outcomes are completely undone in the following year. This also means that we put less structure on the data for the identification of the election effect. To deal with serial correlation of the errors, I will follow the approach suggested by Bertrand et al. (2004) and Kezdi (2002) to cluster the standard errors at the municipality level.

A final specification issue concerns how to control for time effects. Since Swedish local governments have a synchronized fixed election date it is impossible to include a full set of time effects. However, I will include a full set of election period effects instead. Hence, the election effect will only be identified from the variation within election periods. In addition, fixed municipality effects will be added to equation (2) thereby allowing for different growth rates in policy outcomes.

## Step 2

This sub-section tests the more specific predictions for policy choices as derived from the political agency models in section 2. According to implication from these agency models, there are three tests to be made. These tests are displayed in Table 2, while their corresponding predictions across the two types of agency models are presented in Table 3. We can now estimate regression equations of the form

$$(3) \quad \Delta P_{it} = \alpha_0 + \alpha_1 A_{it} + \alpha_2 B_{it} + \alpha_3 C_{it} + \Delta X_{it} \beta + u_{it}$$

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<sup>13</sup> See Wooldridge (2002) for a very good textbook treatment about this subject.

<sup>14</sup> This specification is used by Levitt (1997) in his empirical analysis of electoral cycles in police hiring.

where the definition of the indicator variables  $A_{it}$ ,  $B_{it}$ , and  $C_{it}$  are given in Table 1. With equation (3) we are able to conduct all three tests.<sup>15</sup> For example, the estimate of  $\alpha_3$  would correspond to the POST-ELECTION test.

The empirical analysis will be restricted to the election year and the post-election year since these are the years when the difference in electoral incentives will be most marked. In equation (3), election-period effects will be controlled for, as in equations (1) and (2), as well as allowing for different growth rates in policy outcomes by including fixed municipality effect.

### Step 3

In the third step, electoral success is regressed on policy choices made during the election year since both types of agency models in section 2, has the implication that rational voters should use a retrospective voting scheme, that is, to condition on current policy and to use Bayes rule to update their beliefs about politicians type or effort. Although, policies are endogenous to the chances of being re-elected, there is still some potentially valuable information to be gained by regressing re-election chances on policy. For example, we can test whether spending is positively correlated with electoral success while holding taxes fixed. This test provides information of how voters' value increases in spending in election years controlling for taxes. Hence, this is also a test whether an increase in spending during election years can be interpreted as a signal of competence (as in Rogoff's model) or as a diversion of resources toward private ends (as in Besley and Case/Banks and Sundaram's model).

The following linear probability model will be used to model the re-election decision of a local government  $i$  during an election year  $t$ :

$$(4) \quad R_{it} = c_i + \lambda_t + \Delta P_{it}\omega + \Delta X_{it}\beta + \eta_{it}$$

where  $c_i$  is a municipality effect,  $\lambda_t$  a year effect,  $X_{it}$  are control variables, and  $\Delta P_{it}$  is the growth rate during the election year in the fiscal policy variables.

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<sup>15</sup> Here I have arbitrary chosen  $D_{it}$  as the reference category

## 4. Data

The full sample consists of 288 municipalities between 1974 and 1998. During this period there have been eight elections: 1976, 1979, 1982, 1985, 1988, 1991, 1994, and 1998. In the sample, 2259 observations correspond to election years; 1728 governments were re-elected and 531 were not re-appointed. Table 4 provides more disaggregated information about the number of re-elected and not re-appointed governments each election year.

The classification of changes in power is compiled from the distribution of seats in local councils, which, due to the PR system, is basically equivalent to vote shares. Incumbent governments are classified as left-wing, right-wing or undefined.<sup>16</sup> A government change is defined as a change of power between left-wing, right-wing or undefined governments. This classification of regime changes is quite reasonable in the Swedish context since two main opposing blocs characterize the political map: the left- and right-wing blocs.<sup>17</sup> Moreover, voter approval of incumbent governments is the focus in testing the rational electoral-cycle model, so it seems natural to define a change of power when the incumbent party bloc has lost its majority of votes. Table 5 shows the frequency of government changes for the municipalities. The number of government changes is very unequally dispersed among the different municipalities. For example, 117 municipalities (40 percent of the sample) had no change of power. They had an average vote share of 63 percent. Here it is important to point out that in the tests of the political agency models in step 2, those municipalities with no government turnover will not be part of identifying the electoral cycles in policy since only the within municipality variation will be used.

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<sup>16</sup> The classification is taken from the official newspaper (i.e., [www.kommunaktuellt.com](http://www.kommunaktuellt.com)) of the Swedish Association of Local Authorities. Left wing governments include both the Leftist Party and the Social Democratic Party. Right-wing governments include three parties or more: the Conservative Party, the Centrist party, the Liberal Party, the Christian Democratic Party (since 1988), and the New Democratic Party (1991 to 1994). An undefined government is when neither the left wing nor the right-wing parties constitute a majority (50 percent of the seats) and it is often associated with strong local parties.

<sup>17</sup> Pettersson-Lidbom (2003) presents results that support the view that the Swedish party system can be treated “as if” it is a two-party system at the local level. Moreover, Alesina et al. (1997) also classify Sweden as a bipartisan system (along with U.S. and other political system with a clear left-right division) in their empirical analysis.

The rational electoral-cycle model implies that government spending and taxes are informative about the competence of governments or the amount of rent-seeking. I will therefore use total municipality spending and taxes as dependent variables. Since Swedish local governments only are allowed to raise their revenues through one tax: a proportional income tax,<sup>18</sup> it is possible to use the tax rate itself as a dependent variable. In comparison to the total tax receipt per capita used by many other electoral-cycle studies, the tax rate has the advantage of more closely reflecting of elected governments' intentions. Spending is expressed in per capita terms, using 1991 prices, whereas the tax rate is expressed in percent.<sup>19</sup> Table 6 presents summary statistics for spending and the income tax rate in the sample period. It also presents summary statistics on the control variables: proportion of people of age 0 to 15, proportion of people older than 65, population size, population density, income,<sup>20</sup> and grants-in-aid and aggregate growth. These set of controls are often used in the local public finance literature. For example, including controls for proportions of young and elderly are often rationalized by their link to the cost and benefits of local government spending, while population size and population density are included because they capture the possibility of congestion effects or scale economies in the provision of local government services. Income and grants-in aid are related to the fiscal capacities of municipalities, which may have independent effects on fiscal choices. Finally, I control for common shocks to the macroeconomy by including the percentage change in real GDP.<sup>21</sup>

All the data used are publicly available and were obtained from Statistics Sweden (SCB) or its publications.<sup>22</sup>

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<sup>18</sup> Local governments have the constitutional right to set their own proportional income tax. On average, more than 55 percent of their revenues come from the income tax.

<sup>19</sup> I have used the implicit GDP deflator. The deflator is constructed by taking the ratio of GDP at current market prices to GDP at fixed market prices.

<sup>20</sup> Due to centralization of tax collection, the tax receipts to the local governments in year  $t$  are based on the taxable personal income in year  $t-2$ . In the empirical analysis, I have tried to deal with this feature by including both the municipality income in year  $t$  and  $t-2$  as covariates.

<sup>21</sup> I have also conditioned on the aggregate unemployment rate and the employment rate, and this does not change the results presented below.

<sup>22</sup> The publications used are: How much do local public services cost in Sweden, Local government finance, and Statistical yearbook of administrative districts of Sweden.

## 5. Results

In this section, I present the basic results from the tests of the rational electoral-cycle hypothesis. The presentation of the results will be divided into three subsections each corresponding to one of the steps in the empirical methodology, as explained in section 3. The first step is to test for election effects in policy (i.e., equations 1 and 2), while the second step tests the predictions from the political agency models (i.e., equation 3) and in the third step we regress electoral success on policy (i.e., equation 4).

### 5.1 Basic Results

#### *Step 1*

The results from the test of an electoral cycle in fiscal policy are presented in Tables 7 and 8. The first column in each table shows the results using the fixed-effect estimator (FE) with a lagged dependent variable, the second column shows the results from the instrumental variable estimator proposed by Anderson-Hsiao (AH), the third column, present the results from the first-difference (FD) specification without a lagged dependent variable (i.e., equation 2), and column four shows the results from the FD model with municipality specific effects. A full set of control variables and election-period specific effects are included in all specifications. The results in these tables are striking. Spending is increased and taxes are decreased during election year as compared to off-election years for all specifications. These results are highly statistically significant. The effect on spending is sizable; spending is raised with about 700 to 900 SEK per capita,<sup>23</sup> which is roughly 3 percent of average spending (mean=29,174) or equivalently more than 1 percent of average income (mean=74,934). The size of the effect on the income tax rate is more modest, taxes are lowered in the order of 0.03 to 0.16 percentage points, which is about 0.5 percent of the average proportional income tax rate (mean=16.21). Another observation one can make from these tables is that the results are not particularly sensitive of how one models dynamics, that is, via a lagged dependent variable or through the error term.

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<sup>23</sup> SEK 900 per capita is roughly equivalent to \$ 150 per capita (i.e., SEK 6  $\approx$  \$ 1 in 1991 prices)

## Step 2

The results from the tests of the political agency models is revealed in Tables 9 and 10, As was discussed in section 2, three tests will be performed (see Tables 1-3 for information about these tests): The first test, PRE-ELECTION, is a test of whether the fiscal choices of politicians in the election year differ depending on whether they will be re-elected or not in the upcoming election. The second test, POST-ELECTION, is a test for whether the policy choices of policymakers in their post-election year differ depending on whether they have been re-elected or are newly elected. The third test, PREPOST-ELECTION, is testing whether the post-election year fiscal behavior of re-elected politicians should differ from their election year behavior. Tables 9 and 10 show the results from these tests for spending and taxes, respectively. All specification includes election-period specific effects, while the specification in columns (3) and (4) controls for municipality-specific effects. In columns (2) and (4), a full set of controls is also included.

Table 9 reveals that for spending the PRE-ELECTION and POST-ELECTION tests are positive, while the PREPOST-ELECTION test is negative. All the estimates except one in the table are statistically significant. These results support the implication from Rogoff's model where high spending is a signal of competence. The sizes of these effects are of similar magnitude to the election effect in the previous subsection, that is the order of, 1-3 percent of average spending (mean= 29,174). For example, the PRE-ELECTION test shows that for governments that will be re-elected in the upcoming election increase spending with 400 to 1000 SEK per capita as compared to those which will not be re-appointed.

Table 10 shows the results of the tests for taxes. The results from the PRE-ELECTION test is mixed; it is insignificant in columns 1 and 3 but significantly positive columns 2 and 4 when control variables are added to the specifications. The POST-ELECTION test is negative and significant, while the PREPOST-ELECTION test is positive and significant. Hence, these two latter tests support the predictions from the agency model, whereas the PRE-ELECTION test is basically inconclusive. These results provide further support for the agency models.

### **Step 3**

Table 11 present the results from a regressing electoral success, measured as a binary variable (i.e., re-elected=1, zero otherwise), on the growth of spending and taxes during the election year. Time and municipality fixed-effects are always included in the regressions. In column 2, a full set of controls is also added to the specification. This table reveals that spending growth is correlated with re-election chances, while taxes are not. The insignificant result on taxes is consistent with results for the PRE-ELECTION test in Table 10, namely that there is basically no difference in taxes between governments that will be re-election as compared to those that will not be re-appointed in the next election. The positive correlation between spending growth and electoral success conditional on taxes gives further support to that increases in spending during election year is a signal of competence and not the result from excessive rent extraction.

### **5.2 Robustness checks**

So far I have made the implicit assumption that parties do not matter for fiscal policy choices. However, this statement is not true as shown by Pettersson-Lidbom (2003) using a regression-discontinuity analysis on the same data set. In that paper, I find strong evidence that left-wing parties spend and tax more than right-wing parties. Hence, this party effect may be confounded with the electoral-cycle effects in fiscal policies. To test whether this is the case, all the results from the three-step methodology steps is remade by including the same variables as in Pettersson-Lidbom's regression-discontinuity analysis, namely indicators for left- and right-wing governments and together with vote shares. These results are presented in Tables 12-14 and they reveal that all the previous results are unchanged.



## **6. Discussion and conclusion**

This paper develops a three-step empirical approach to assess the empirical relevance of the rational electoral-cycle hypothesis using an attractive data set from Swedish local governments with more than 2000 observation from elections. The results are striking. In election years, spending is increased and taxes decreased. Moreover, governments that are about to get re-elected raise spending as compared to governments about those that will be replaced in the upcoming election. Furthermore, in the post election year, re-elected governments have higher spending and lower taxes than newly elected ones. Another finding is that re-elected governments have lower spending and higher taxes in the post-election than in the election year. Finally, electoral success is positively correlated with spending holding taxes fixed. These results provide strong support to Rogoff's model where government signals their competence trough cycles in fiscal policy.

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Table 1. Definition of indicator variables

A	=1 if <u>election</u> year and the government will be <u>re-elected</u> in the upcoming election =0 otherwise
B	=1 if <u>election</u> year and the government will be <u>replaced</u> in the upcoming election =0 otherwise
C	=1 if <u>post-election</u> year and the government is <u>re-elected</u> in the election =0 otherwise
D	=1 if <u>post-election</u> year and the government is newly elected =0 otherwise

Table 2. Definition of the tests of the agency models

PRE-ELECTION	$E[Policy   A=1] - E[Policy   B=1]$
POST-ELECTION	$E[Policy   C=1] - E[Policy   D=1]$
PREPOST-ELECTION	$E[Policy   C=1] - E[Policy   A=1]$

Table 3. Predictions about fiscal policy from the agency models

TESTS	Spending	Taxes
<i>Rogoff</i>		
PRE-ELECTION	Positive	Negative
POST-ELECTION	Positive	Negative
PREPOST-ELECTION	Negative	Positive
<i>Besley and Case/Banks and Sundaram</i>		
PRE-ELECTION	Negative	Negative
POST-ELECTION	Negative	Negative
PREPOST-ELECTION	Positive	Positive

Table 4. Number of reelected and not re-elected governments each election year

Election year	Re-elected	Not re-elected
1976	233	44
1979	237	40
1982	229	50
1985	244	40
1988	237	47
1991	179	105
1994	144	142
1998	225	63
Total sum 1976-1998	1728	531

Note. - A non re-elected government is defined as a change of power between left wing, right wing or undefined governments.

Table 5. Frequency of government turnovers

Frequency of government turnovers	Number of municipalities
0	117
1	26
2	43
3	39
4	32
5	16
6	11
7	4
8	0

Note. - A government turnover is defined as a change of power between left wing, right wing or undefined governments.

Table 6. Descriptive statistics for the dependent and control variables

Variables	Mean	Standard d.	Min	Max
Total spending	29,174	6,015	14,392	70,032
Income tax rate (%)	16.21	1.82	9.7	32.25
Aggregate growth of the Swedish economy	1.65	1.70	-2.2	4
Proportion of young, 0-15	21.05	2.69	12.65	36.69
Proportion of old, 65+	17.79	4.22	3.27	28.14
Income	74,934	13,302	15,944	174,473
Population size	29,923	53,074	2,865	727,339
Population density	115	373	0.28	3,884
Grants	2,589	2,598	-4,749	19,599

Spending, income and grants are expressed in per capita terms and in 1991 prices.

Table 7. Electoral cycles in spending

	1	2	3	4
Election year effect	917 (66)*	886 (89)*	737 (75)*	727 (78)*
Lagged dependent variable	Yes	Yes	No	No
Election-period effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Fixed effects	-	-	No	Yes
Number of observations	6750	6150	6750	6750

Notes- Standard errors within parentheses; In columns 1 and 2, Huber-White robust standard errors are used to compute standard errors, while in columns 3 and 4, Huber-White Standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are used compute standard errors: \* significant at 1%; \*\* significant at 5%, \*\*\* significant at 10%. In columns 1 and 2, the following regression is run  $Spending_{it} = \mu_i + \delta ELE_{it} + \gamma Spending_{i,t-1} + X_{it}\theta + \varepsilon_{it}$ . In column 1, a fixed effect estimator is used, whereas in column 2 the instrumental variable estimator proposed by Anderson and Hsiao is used with  $SpendingP_{i,t-2}$  as instrument for  $SpendingP_{i,t-1}$ . The following control variables is being used: aggregate growth, proportion of people of age 0 to 15, proportion of people older than 65, population size, population density, income, income (t-2), and grants-in-aid

Table 8. Electoral cycles in taxes

	1	2	3	4
Election year effect	-.031 (.007)*	-.168 (.040)*	-.080 (.008)*	-.080 (.008)*
Lagged dependent variable	Yes	Yes	No	No
Election-period effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Fixed effects	-	-	No	Yes
Number of observations	6777	6201	6777	6777

Notes- Standard errors within parentheses; In columns 1 and 2, Huber-White robust standard errors are used to compute standard errors, while in columns 3 and 4, Huber-White Standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are used compute standard errors: \* significant at 1%; \*\* significant at 5%, \*\*\* significant at 10%. In columns 1 and 2, the following regression is run  $Taxes_{it} = \mu_i + \delta ELE_{it} + \gamma Taxes_{i,t-1} + X_{it}\theta + \varepsilon_{it}$ . In column 1, a fixed effect estimator is used, whereas in column 2 the instrumental variable estimator proposed by Anderson and Hsiao is used with  $\Delta Taxes_{i,t-2}$  as instrument for  $\Delta Taxes_{i,t-1}$ . The following control variables is being used: aggregate growth, proportion of people of age 0 to 15, proportion of people older than 65, population size, population density, income, income (t-2), and grants-in-aid

Table 9. Political agency: spending

Dependent variable: $\Delta$ Spending				
	1	2	3	4
PRE-ELECTION	826 (134)*	388 (124)*	1011 (156)*	495 (142)*
POST-ELECTION	203 (134)	220 (120)***	384 (167)**	321 (147)**
PREPOST-ELECTION	-664 (89)*	-1068 (101)*	-663 (92)*	-1056 (108)*
Election-period effects	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Fixed effects	No	No	Yes	Yes
Number of observations	4217	4217	4217	4217

Notes- Standard errors within parentheses; Huber-White Standard errors allowing for clustering at the municipality level to account for possible serial correlations in the errors within municipalities are used compute standard errors: \* significant at 1%; \*\* significant at 5%, \*\*\* significant at 10%. In column 1 the following regression is run:  $\Delta\text{Spending}_{it} = \alpha_0 + \alpha_1 A_{it} + \alpha_2 B_{it} + \alpha_3 C_{it} + u_{it}$  and set of controls  $\Delta X_{it}$  are added to the specification column 2. In column 3 the following regression is run:  $\Delta\text{Spending}_{it} = \mu_i + \alpha_0 + \alpha_1 A_{it} + \alpha_2 B_{it} + \alpha_3 C_{it} + u_{it}$  and a set of controls  $\Delta X_{it}$  are added to the specification in column 4. The following control variables is being used: aggregate growth, proportion of people of age 0 to 15, proportion of people older than 65, population size, population density, income, income (t-2), and grants-in-aid



Table 10. Political agency: taxes

Dependent variable: $\Delta$ taxes				
	1	2	3	4
PRE-ELECTION	.011 (.010)	.022 (.010)**	.014 (.012)	.028 (.012)**
POST-ELECTION	-.056 (.015)*	-.046 (.015)*	-0.56 (.017)*	-.042 (.017)**
PREPOST-ELECTION	.024 (.009)*	.015 (.009)***	.023 (.009)**	.015 (.010)
Election-period effects	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Fixed effects	No	No	Yes	Yes
Number of observations	4230	4230	4230	4230

Notes- Standard errors within parentheses; Huber-White Standard errors allowing for clustering at the municipality level to account for possible serial correlations in the errors within municipalities are used compute standard errors: \* significant at 1%; \*\* significant at 5%, \*\*\* significant at 10%. In column 1 the following regression is run:  $\Delta \text{Taxes}_{it} = \alpha_0 + \alpha_1 A_{it} + \alpha_2 B_{it} + \alpha_3 C_{it} + u_{it}$  and set of controls  $\Delta X_{it}$  are added to the specification column 2. In column 3 the following regression is run:  $\Delta \text{Taxes}_{it} = \mu_i + \alpha_0 + \alpha_1 A_{it} + \alpha_2 B_{it} + \alpha_3 C_{it} + u_{it}$  and a set of controls  $\Delta X_{it}$  are added to the specification in column 4. The following control variables is being used: aggregate growth, proportion of people of age 0 to 15, proportion of people older than 65, population size, population density, income, income (t-2), and grants-in-aid

Table 11. Effects on policy on re-election chances

Dependent variable: Indicator for re-election		
	1	2
$\Delta$ spending	8.44e-06 (3.64e-06)**	8.71e-06 (3.75e-06)**
$\Delta$ taxes	.014 (.035)	.017 (.035)
Time effects	Yes	Yes
Fixed effects	Yes	Yes
Controls	No	Yes
Number of observations	2258	2258

Notes- Standard errors within parentheses; Huber-White Standard errors allowing for clustering at the municipality level to account for possible serial correlations in the errors within municipalities are used compute standard errors: \* significant at 1%; \*\* significant at 5%, \*\*\* significant at 10%.. The following control variables is being used: aggregate growth, proportion of people of age 0 to 15, proportion of people older than 65, population size, population density, income, income (t-2), and grants-in-aid

Table 12. Step 1 with party controls

	1	2	3	4
Spending				
Election year	912 (66)*	885 (86)*	735 (75)*	727 (77)*
Taxes				
Election year	-.032 (.007)*	-.169 (.035)*	-.080 (.008)*	-.080 (.008)*

See the notes from Tables 7 and 8.

Table 13. Step 2 with party controls

	1	2	3	4
Dependent variable: $\Delta$ Spending				
PRE-ELECTION	915 (135)*	462 (125)*	1039 (158)*	510 (142)*
POST-ELECTION	320 (148)**	317 (129)**	435 (176)**	357 (152)**
PREPOST-ELECTION	-658 (89)*	-1064 (101)*	-657 (92)*	-1050 (108)*
Dependent variable: $\Delta$ Taxes				
PRE-ELECTION	.0085 (.011)	.020 (.011)***	.0099 (.012)	.024 (.013)***
POST-ELECTION	-.059 (.015)*	-.048 (.015)*	-.059 (.017)*	-.045 (.017)*
PREPOST-ELECTION	.024 (.009)**	.015 (.009)***	.023 (.010)**	.015 (.010)

See the notes from Tables 9 and 10.

Table 14. Step 3 with party controls

Dependent variable: Indicator for re-election		
	1	2
$\Delta$ spending	9.17e-06 (3.62e-06)**	9.08e-06 (3.85e-06)**
$\Delta$ taxes	.011 (.034)	.012 (.035)

See the notes from Table 11.